homology of the *trabeculæ cranii*, most of what is stated in this chapter is well enough established to form part of a manual for students of comparative anatomy.

But who are these students? No one could follow the closely printed pages of description here given, without a good general acquaintance with human anatomy and a thorough knowledge of the human skeleton. For this reason we think it would have been better to have curtailed or even omitted the preliminary accounts of each organ in man, because they are not sufficient alone, and there are many excellent treatises on this subject already. If it is answered that the book is really intended for boys and girls at school, then the details given, especially in osteology, are far too numerous: in fact they would be unintelligible without a good museum, and learning zygosphenes and hypapophyses without seeing them is far worse mental training than *Barbara Celarent*, or the verbs in -μι. For the second class of readers mentioned in the preface, teachers, medical students, and others acquainted with human anatomy, this little treatise will be found just what they want in order to learn "its more significant relations to the structure of other animals." The only defect they will find is the omission of the organs of reproduction and the structure of the ovum.

The woodcuts are generally sufficient, and some of the diagrams are remarkably ingenious and useful. Some are, however, much too small, *e.g.* the diagram of the skull, Fig. 197, and all the figures of entire skeletons, as 200; while others, as 137, representing the shoulder-girdle of *Hemidactylus*, after Parker, greatly need the shading and tinting of the original drawing. The plan of repeating an illustration whenever it is referred to is not often adopted in English books, but on the whole it is, we think, the most convenient.

Experience will show what class of students will really make most use of Mr. Mivart's Lessons. We heartily recommend them to all medical students and zoologists who have access to a good museum. P. S.

OUR BOOK SHELF

Die Robbe und die Otter (Phoca vitulina *et* Lutra vulgaris) *in Ihrem Knochen-und Muskel-skelet.* Eine anatomisch-zoologicgische Studie von Dr. J. C. G. Lucae. 102 pp. 15 plates. (Frankfort-on-the-Main, 1873.)

UNDER this title the distinguished anatomist, Prof. Lucae, has contributed to the "Transactions of the Senckenbergian Society of Naturalists," an elaborate treatise upon the anatomy of the Common Seal (*Phoca vitulina*). The osteology of *Phoca* is minutely described, and every part of its skeleton compared with the corresponding portions of that of the Otter—one of its nearest allies among the terrestrial Carnivora. Comparisons with other mammals are also given.

Fifteen well-executed plates illustrate this excellent memoir, which, when completed (the first part being only now before us) will leave little to be added to our information as to the osteology of the true Seals (*Phocidæ*.) To our knowledge of the structure of the two other families of the marine Carnivora (the Trichecidæ and Otariidæ) we have lately received a valuable contribution in the shape of Dr. Murie's Memoirs on the Walrus and Sea-lion, published in the Zoological Society's "Transactions," so that great progress has lately been made towards a perfect understanding of the osseous structure of the marine Carnivora.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Agassiz and Forbes

MR. GEORGE FORBES has, in NATURE of May 22, given his version of the controversy between Agassiz and Forbes. I had no intention, in a former note, of reviving, for the benefit of the readers of NATURE, this unpleasant subject; but simply wished to protest against the *ex cathedra* statements of the reviewer of Tyndall. The materials for an impartial discussion of the history of glacier work are accessible to all investigators, and when it comes to be written, Agassiz and Forbes will obtain due credit for their share of the work. One of the points at issue between Forbes and Agassiz is not a matter "of facts to be proved or disproved by facts." The conversation between Agassiz and Forbes (Heath as witness for Forbes) held on the first day of their sojourn on the Aar Glacier, refers simply to *a difference of opinion on the explanation of certain bands* (observed previously by several persons, and well known to Agassiz). The nature of these bands has to this day remained problematical. and why Agassiz, when writing to Humboldt that *he had observed these bands at a depth of* 120 *ft. in the body of the glacier*, should give any credit to Forbes passes all understanding. This observation was made after Forbes's departure, and Agassiz certainly needed no "reconciliation with his conscience" to describe this as "le fait le plus nouveau que *j'ai* observé." The testimony of Mr. Heath is of no value, for it certainly would be the height of presumption, in a man without any previous acquaintance with glaciers, to undertake to decide in a few lines, a point to this day a subject of controversy among investigators of glaciers; his endorsement of the claims of Forbes is as ridiculous as the attempt made by a prominent Swiss geologist (who gives his testimony in favour of Forbes), to ignore the claims of Agassiz, by passing his name over in silence when writing the history of geological science in Switzerland.

I would also remind Mr. George Forbes and the editors of the "Life and Letters of Forbes," that Agassiz's affirmation carries as great weight as that of Forbes or Mr. Heath. Forbes is entitled to whatever credit there is in his explanation of these bands and no more, an explanation which has not been adopted by Agassiz for the very good reason that he did not deem it a satisfactory one, and did not attach to it the same importance which Forbes did. Agassiz expressed to Forbes considerable surprise at the appearance of the bands which presented that morning peculiar conditions, not usually seen except after a hard rain, and on the strength of this surprise Forbes lays claim to the discovery of the bands, and boldly accuses Agassiz of knowing nothing about them at the time of his visit. In investigations carried on for several years, as those of the glacier of the Aar under Agassiz, it was most natural that special points should not always be uppermost in the mind of the investigator, however interesting they might appear to a visitor; this will fully account for any want of interest shown by Agassiz on first meeting Forbes and discussing the veined structure of the ice.

Agassiz certainly owed nothing to Forbes, who was an invited guest on the glacier of the Aar, a novice in glacial work. No attack was made upon Forbes, as is stated by Mr. George Forbes; it originated with him. In a letter addressed to Forbes by Agassiz when he first discovered that Forbes had published, independently as his own, observations made upon the glacier of the Aar, during his stay with the Swiss party, he says: "the idea that in thought you conceived the project of an independent publication did not come to me for an instant. I should have thought I did you injustice by such a supposition." Agassiz felt *he* "had been deeply wronged" by the course taken by Forbes; he made no answer to Forbes, and paid no further attention to the subject, not because there was "no room for discussion," but because the tone adopted by Forbes was so insulting and overbearing as to render all further discussion impossible without its degenerating into the personalities afterwards indulged in by Forbes, in his letters to his friends, which the editors of his Life and Letters have taken special pleasure in reproducing.

Forbes did not hesitate to bring Mr. Heath uninvited to the glacier of the Aar, probably to act as his witness and swel the party, yet both he and his son regard the presence o friends of Agassiz, to assist him in his work, a most monstrous circumstance. Pioneers usually find it difficult to explore the

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way, but when the track is once blazed it is easy enough to follow and find the path.

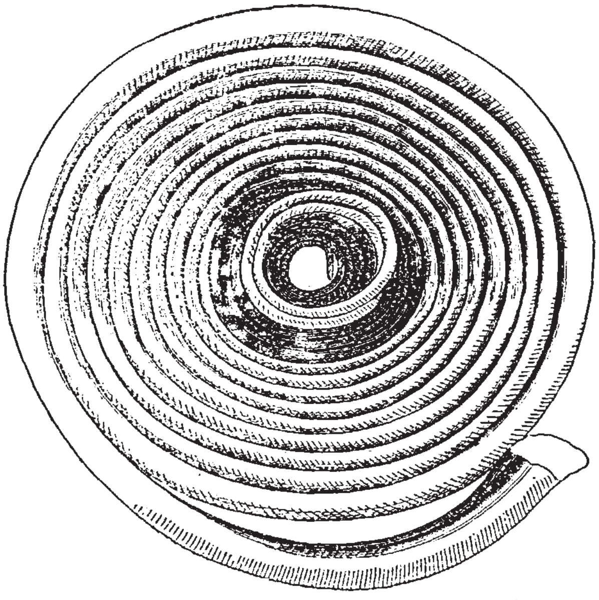
As I do not wish to fill the pages of this journal with personal explanations, my contributions in NATURE to this subject must cease with this note. It is not my purpose at present to refute the imputations cast upon Agassiz by the editors of Forbes's Life and Letters; he can well afford to pass them over as he has done thus far, in silent contempt, the more so since, fortunately for Agassiz, the editors have given us from Forbes's own letters all that was necessary to show a course of duplicity, on Forbes's part, towards the man with whom "he served his apprenticeship in glacier observation," which is happily rare among scientific men. ALEXANDER AGASSIZ

Probosces capable of sucking the Nectar of Anagræcum sesquipedale

MR. W. A. FORBES, in the number for June 12 started the question, whether moths are known to inhabit Madagascar with probosces capable of such an expansion, as to obtain the last drops of the nectar secreted in the lower part of the whip-like nectaries of *Anagræcum sesquipedale*.

As long as a direct answer to this question has not been given, it may be of some interest to state in general the existence of moths provided with probosces sufficiently long for the honey-spurs in question.

Some days ago I received a letter from my brother, Fritz Müller (Itajahy, Prov. St. Catharina, Brazil), in which he says: "I recently caught a Sphinx (not determinable by Burmeister's "Brazilian Sphingidæ"), the proboscis of which has a length of about 0·25 metres—a length not approached by any honey-tube of this country known to me. I enclose the proboscis." Being unable to get the name of this species of Sphinx, I append the illustration of its proboscis, magnified in the proportion 7 : 1.



This proboscis, in its contorted condition forming a roll of 10-11 millimetres in diameter, and showing at least 20 elegant windings, in its expanded condition attains a length of between 10 and 11 inches, and would consequently be adapted to the nectaries of *Anagræcum sesquipedale*, which have been found by Darwin 11½ inches long, with only the lower inch and a half filled with nectar. Darwin indeed says, with regard to the fertilisation of *Anagræcum sesquipedale* (p. 198 of his work on Orchids): "there must be moths with probosces capable of extension to a length of between 10 and 11 inches."

Lippstadt, July 1 HERMANN MÜLLER

An Order of Merit

YOUR leading article in the last number of NATURE on the subject of a proposed "Order of Merit for Scientific Men," recalls the views (in exact correspondence with your own) entertained by my brother-in-law, the late J. Beete Jukes. These were expressed by him in no uncertain terms on the occasion of publishing an address on the Geological Survey, delivered in Dublin in 1865.

I take the liberty of sending you a print for your perusal, and to refer to note B, at p. 21. I was glad to see the subject so well dealt with in your article. ALF. H. BROWNE

5, West Hill, Highgate

"Men of science have of late years pandered too much to the utilitarian quackery of the age, and it is time that some one should stand up to protest against it. Government and the House of Commons should be told that Science must be supported and encouraged for her own purely abstract purposes, independently of all utilitarian applications. The necessary preliminary, indeed, to these utilitarian applications is the discovery and establishment of abstract scientific truth by men who look to that alone, and whose whole faculties and lives are devoted to it. The men who afterwards make the practical applications of it often attain, indeed, far wider reputations than the real men of science, and become to the popular gaze the representatives of Science itself. The higher class are rarely much known to the public during their lives, and are not usually men who would experience any satisfaction if they were nick-named Knights or labelled with C.B., or would feel inclined to accept any other crumbs that might fall from the table of the politically great and powerful. Nor would they commonly care much for pecuniary rewards, unless as a means to enable them to do their work without drudging for the support of themselves or their families. They are the men, however, who in the end rule the world, and doubtless they are often sustained in their labours by a consciousness of this fact.

"It would manifestly conduce to the public good and the national honour if such men, when they do arise amongst us, should be sought out, recognised as public benefactors, and allowed means to do that work which their faculties, and theirs only, enable them to perform." ("Her Majesty's Geological Survey of the United Kingdom," &c., by J. Beete Jukes, F.R.S. 1867.)

Geological Subsidence and Upheaval

SIR J. HERSCHEL thought that the earth's crust floats upon an ocean of molten matter, and that the washing of detritus from the land into the sea, by altering the relative weight of different portions of the shell, occasions a subsidence of the ocean's bed and an upheaval of the land, which may be either gradual and insensible, like the process of denudation, or spasmodic and by fits and starts producing earthquakes and sometimes volcanic eruptions.

This theory was at one time adopted, at least partially, by Sir C. Lyell, but is not mentioned in the latest edition of his "Principles," and is generally rejected by geologists as at variance with the opinion held by Sir W. Thomson and others in regard to the internal solidity of the earth. But this objection may be avoided by modifying Sir J. Herschel's theory. We may repudiate his hypothesis that a great fiery ocean exists below the outer crust. We may arrive at many of the important conclusions which he drew from this hypothesis, and which he described as all that a geologist could require, by admitting either that *solid rocks are plastic*, or that *some of the lower and warmer strata of the earth are more pliable than the upper*.

As to the plasticity of solid bodies, it may be sufficient to refer to the experiments of M. Tresca (Comp. Rend. de l'Acad., 1864-65, and Annales du Conservatoire, No. 21). Dr. Tyndall (Glaciers of the Alps, p. 9) suggests the possibility that the contortions of the strata in the valley of Lauterbrunnen may have been produced by pressure acting throughout long ages on the rocks in their present hard and solid condition.

Again, the lower strata of our globe may be rendered more pliable than the superincumbent rocks by the great internal heat, although it may be insufficient to fuse them or even to maintain them in a viscous condition. Many of the geological effects of a molten ocean may thus be produced.

The theory that volcanic eruptions are caused by water percolating through superficial cracks may, perhaps, give a clue to the reason why volcanoes often occur in a great circle round the globe and in diametrically antipodal positions. When other causes concur to modify the form of the earth, the tidal strain occasioned by the sun and moon may often be required to overcome the *vis inertiæ;* this strain being greatest in the great circles of the globe perpendicular to the direction in which the sun and moon happen to be, cracks would probably occur most readily in these circles.

It seems at least a curious coincidence that some areas of recent

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